theory. Nor do I; on the contrary, I regard it as an important aid to the general application of the Darwinlan theory.

Apart from the objection, which I have above expressed, to the treatment of this question by Mr. Dyer as one of orthodoxy, I note with regret that he has (unintentionally no doubt) misrepresented what I have actually said in my letter. Whereas I have expressly cited Mr. Darwin's principle of "correlation of variations," Mr. Dyer writes of the "extended correlation principle of Prof. Lankester." This form of pleasantry could be itself extended, but more of it would be unworthy of your pages. As a matter of fact, what I said as to correlation was very little more than a citation of cases and their theoretical explanation given by Mr. Darwin. To represent this as in any way parallel to the independent and anti-Darwinian theories of Mr. Romanes and Mr. Bateson, as is done by Mr. Dyer, is misrepresentation. The theory of Wells as to the black races of the tropics was used by me in order to illustrate my suggestion as to correlation leading to a development of useless specific characters. I might, equally well for my purpose, have used any of the other cases collected by Mr. Darwin. Whilst I spoke of Wells' case as "striking and suggestive," I at the same time expressly referred to it as "a more or less hypothetical case." Mr. Dyer has no justification, so far as I can see, for stating that I use this case as a foundation for wide generalisation. I made no wide generalisation, but adduced the wide generalisation at which Mr. Darwin, after collecting and considering a large variety of cases, arrived, viz. that correlation of variation does occur largely and generally in the organic world. This wide generalisation is, I say, not mine; it is Mr. Darwin's. If this generalisation be accepted—and we may reasonably hope that the apostolic and orthodox do accept it—then it seems to me in the highest degree probable that an obscure specific difference of structure—highly effective as life-preserving or progeny-ensuring—will more or less frequently carry with it as a correlated variation a more obvious and measurable character in some remote part of the body, not effective, that is to say, not useful. Hence I conclude that there may be specific characters (not by any means all or always) which are not themselves *useful*, though readily observed. That is the whole of my contribution to the present discussion. It does not seem to me to involve anything rash or surprising, though such is its character according to Mr. Dyer.

I may remind the readers of NATURE that some years ago, in these columns, I adduced this same principle, viz. Mr. Darwin's principle of correlation of variations, as one sufficient to remove some of the difficulties in the way of the doctrine of natural selection brought forward by the Duke of Argyll. What the Duke called "prophetic germs" might, it seemed to me (and still seems to me), when not explicable as lapsed rudimentary structures, be accounted for as variations or new structures correlated with other useful and therefore selected variations, although not yet themselves useful. A useless variation correlated with a useful one must (it seems to me) be supposed to pass through initial stages in which it is too small, or otherwise insignificant, to be useful (its utility or harmfulness being swamped in the utility of the correlated useful character), and only after attaining considerable development becomes either useful or harmful, and therefore subject to selection, possibly under some slight change of environment.

I regret all the more my differences with my friend Mr. Dyer, because the explanations they have involved leave me so little space in which to refer to Prof. Weldon's courteous and interesting letter. I have only one point to correct in his statement of my position in relation to that taken by him. The attempt to reconcile the *dicta* of Hume or Kant, or even of Mill, with the experience and approved practice of those who make it their business to investigate natural phenomena, would be an interesting undertaking, possibly one beyond the powers of living man. It is certainly not one upon which I shall here and now embark.

The point whereon Prof. Weldon has misunderstood my contention is this. After describing a phenomenon (death-rate) preceded invariably by two or more phenomena of structure or function, he says: "Under these circumstances, Prof. Lankester thinks it legitimate to pick out one of these antecedent phenomena and to speak of it as the only effective cause of change in death-rate, the other phenomena, although equally universal, being merely unimportant concomitants of this one essential change." Prof. Weldon is mistaken in stating that I think it legitimate "to pick out" without qualification, or at haphazard, any one of these antecedent phenomena, and to speak of it as the only effective cause. I should object to such a proceeding on much the same ground as that on which I object to his calling "any and all" of those antecedent phenomena effective causes.

What I think is the reasonable course in such a case—supposing that a man wishes to ascertain, as fully as may be, the relation of these phenomena to one another, is that he should frame in his mind a hypothesis as to how any one or more of the phenomena, invariably associated with a given death-rate, can operate so as to effect an increase or decrease in that death-rate. This, no doubt, will require a large knowledge of the surrounding conditions not usually to be acquired with ease, and an analysis of the antecedent phenomena in question, often of a prolonged and laborious character. Sometimes, however, such a hypothesis will present itself very readily and with much antecedent probability. However attained, the hypothesis will remain merely a guess until it is tested. It can be tested either by experiment or by observation of appropriate natural instances. By repeated testing, involving often great ingenuity and prolonged labour, the hypothesis is either confirmed or discarded; possibly a new hypothesis is adopted, so to speak, *en route*, and established as in all probability true. When—and not until—this process has been gone through, the naturalist will be, more or less according to the extent of his work, in a position to *place* the phenomena in their true relation to one another and to the ultimate phenomenon proposed for investigation, viz. death-rate. If the study of the antecedent or associated phenomena by means of hypothesis and test-experiment has not been and cannot be carried out, the naturalist can not (it seems to me) reasonably either "pick out" one of them and assert that it is the cause of increased or decreased death-rate, nor (still less) declare that all the antecedent or associated phenomena are the causes and produce the effect of increased or decreased death-rate. If he does do so, he appears to me to be evading the task before him, which is to "explain," that is, to place in their true order and relation a complex group of related phenomena.

The appeal to analogy no doubt frequently leads naturalists to rapid conclusions as to the causal relations of the phenomena of organisms. Often (but by no means always) such conclusions are erroneous: but it is not on that account desirable to reject the argument from analogy in reasoning about such matters. It must be used with knowledge and with caution. It seems to me, that in considering a complex case in which actual experiment is as yet wanting, it is often more useful to formulate provisional conclusions as to what is cause and what only concomitant effect by the aid of argument from analogy, than to deliberately reject all attempts at analysis, and to "lump" all the constantly associated phenomena as "causes." Surely in the case of Prof. Weldon's crabs, most naturalists would take the view that the frontal measurements may *possibly* be operative in saving the life of the crab, or *may* be only a correlative of some other life-preserving structure; that its quality in this respect should be inquired into by means of hypothesis and experiment; and that, until this is done, it is *premature* to speak of a particular frontal proportion as having for its *effect* the survival of those crabs distinguished by its possession.

The chief task of the student of living things seems to me to lie in the search for such explanations, even though the task is in some cases to all appearance at first sight hopeless, and even though too hopeful and imaginative spirits may be led, not unfrequently, to propound explanations which are insufficiently supported by observed facts, or are demolished by the observation of other facts. You can not (it seems to me) reduce natural history, as Prof. Weldon proposes, to an unimaginative statistical form, without either ignoring or abandoning its most interesting problems, and at the same time refusing to employ the universal method by which mankind has gained new knowledge of the phenomena of nature—that, namely, of imaginative hypothesis and consequent experiment. I think that most naturalists will agree with Johannes Müller that "Die Phantasie ist ein unentbehrliches Gut." E. RAY LANKESTER.

Dinard, Bretagne, August 10.

Habits and Distribution of Galeodes.

WILL you be good enough to publish in your widely-read paper the following notes on the geographical distribution of *Solfuga* (*Galeodes*) *araneoides*, as the published accounts of that arachnid are incomplete in that respect.

© 1896 Nature Publishing Group

In India it is found in the central parts of India, in the Punjab, in Afghanistan, in Baluchistan. Probably it will be found in Rajputana and Scinde, and perhaps in some parts of the Dekhan. I have never found it in the alluvial plains of Northern India, nor in Bengal. I have just found it in the Eastern Sudan. Probably it exists in most of the deserts of Asia and Africa. The ones I have found all seem to belong to one species. They are of a light straw colour, often with some black above. A black variety is found on the Afghan frontier. The biggest specimen measured 1⅝ inches from the tips of the jaws to the end of the abdomen. They feed on soft insects like moths, and live in holes on the ground. They are very rapid in their movements, and are difficult to catch when on the move. The best way to get them is to put a glass over them when they are lying still in a corner. A big match-box often acts as a good trap. They will run inside it of their own accord. They go up walls, posts, trees, in search of insects, and will jump down from a fair height.

The movements of their jaws when feeding presents a unique sight. The head is small, and the bases of the jaws are bulbous and look like a continuation of the head. When eating they move their jaws alternately, and one gets the impression that the head is jointed and that each side moves alternately. In encounters with spiders, scorpions and centipedes, they usually fare badly. Their belly is so soft, that once caught there, they are done for. Still they are very combative.

I have often tried to settle the point as to whether their bite is painful or poisonous. But I never could succeed in making one bite a human being. E. Cretin.

Suakin Force, July 12.

[The genus Galeodes, represented by several species, is known to extend in Africa from Algeria to Egypt, thence southwards into Somaliland; in Europe, from Greece throughout the steppes of South Russia; in Asia, over the whole of Asia Minor, Arabia, Persia, Baluchistan, Afghanistan and Turkestan, and thence into India, where it has been recorded from the following localities: Punjab (Kohat), Rajputana (Bikanir), Gwalior, Delhi, Secunderabad, Guntakul near Bellary, Birhbum, Bengal and Madras.—R. I. Pocock.]

Nest-building Amphipod in the Broads.

It will be remembered that in 1891, *Cordylophora lacustris* was found in great abundance in Heigham Sound. On the 6th inst,, after collecting in that locality and downward to Potter Heigham Bridge, I noticed at the bottom of the bottle a Crustacean, apparently a Podocerid. On floating a piece of reed stem, covered with colonies of the hydrozoon, in a vessel of water, it was evident that the colonies were thickly studded with nests, from which, in some cases, the antennæ were seen protruding. Several specimens of these Amphipods were secured at once; and these the Rev. T. R. R. Stebbing, F.R.S., has kindly identified. They prove to be *Corophium crassicorne*, Bruzelius. A large colony of the Cordylophora has been preserved in formalin, with the nests. On the following day I met with the same Amphipod, in considerable numbers, between Acle Bridge and the Angel Inn. Henry Scherren.

9 Cavendish Road, Harringay, N.

The Effects of a Strong Magnetic Field upon Electric Discharges in Vacuo.

Some interesting experiments upon the subject of this note were described in Nature, July 19. A small addition to those experiments serves to show the connection between the electric conductivity of the tube and the mean free-path-length of the included molecules.

Using the same form of Crookes' tube as employed by Mr. Swinton, but provided with the platinum Maltese cross to intercept the kathode rays, the shadow of the cross is rotated upon its centre through an angle depending upon the strength of the magnetic field. The motions therefore of the molecules are changed from right lines to spiral lines, and thus their free paths are lengthened—a result equivalent to a further exhaustion of the tube. The primary effect of the magnetic field, observed by Mr. Swinton, was an increase of conductivity in the tube. The two experiments therefore show that the conductivity in the tube increases with lengthening of the free paths.

By lowering the tube slowly from a few inches above the pole of the electro-magnet down to contact, the shadow of the cross is seen to rotate slowly, and to become smaller as the luminescent cone contracts to the form shown by Mr. Swinton, with the apex on the bulb.

The rotation of the shadow is reversed by reversing the poles of the magnet; and when the current is reversed in the tube, the green fluorescence appears as a spiral band round the walls of the tube—a right-handed or left-handed helix, according to the polarity of the magnet. Walter Sidgreaves.

Stonyhurst College Laboratory.

THE LIVERPOOL MEETING OF THE BRITISH ASSOCIATION.

The detailed local arrangements are now progressing rapidly. St. George's Hall, where the reception room and offices will be situated, has just been re-painted and decorated by the Corporation at great cost. The old wooden flooring has been removed so as to expose the beautiful tiled pavement, which has not been seen for many years, and which will not be covered up again until after the meeting. The buildings and rooms allotted to the various Sections were mentioned in the former article. The sectional fittings have now been planned out, and will be commenced at once. Separate electric and oxy-hydrogen lanterns will be provided for all the Sections that desire them. A large lecture theatre, holding over six hundred people, at University College, will be available for joint discussions between the Sections.

The details of most of the excursions have now been arranged, and a special "Excursions Guide," in addition to the "Handbook," has been prepared. Amongst the excursions, that to the Isle of Man at the conclusion of the meeting will probably take a foremost place, both on account of its general attractiveness and of its special scientific interest. This excursion will extend over five days—from Thursday, September 24, to Monday, September 28, inclusive; and the party will break up into four Sections: (1) Archæologists, (2) Geologists, (3) Zoologists, and (4) Botanists, to be conducted by competent leaders over those parts of the island which offer special attractions for scientific study. The geology of the island is varied and interesting, especially as regards the dynamic alteration of the older Palæozoic rocks, the volcanic series and the richly fossiliferous limestones of Carboniferous age, and the wide developments of the glacial deposits; the Prehistoric, Scandinavian, and other early remains are celebrated; the marine fauna and flora are abundant, and the presence of the Liverpool Marine Biological Station at Port Erin affords facilities for dredging expeditions and other biological work.

The detailed programme for the four Sections, which follows, has been arranged by a Committee of the Isle of Man Natural History and Antiquarian Society, acting along with representatives appointed by the Liverpool Executive Committee; and a special appendix to the Liverpool "Handbook," containing an account of the geology, antiquities, and natural history of the island, illustrated by a geological map and a chart, has been drawn up by Mr. P. M. C. Kermode, Mr. G. W. Lamplugh, and Prof. Herdman.

Section I.—Archæologists.

Leaders: Arthur J. Evans, P. M. C. Kermode.

Thursday, 24.—Arrive by steamer from Liverpool about 3 p.m. Reception by his Excellency the Right Hon. Lord Henniker, Lieut.-Governor, at Government House. Headquarters at Douglas.

Friday, 25.—Carriages at 9.30 a.m. for Braddan (see ancient crosses and alignments), St. Trinian's, then Tynwald Hill (see mound, cist, Runic cross, &c.). Lunch at Creg Malin Hotel, Peel, at 1 p.m. See Peel Castle, round tower, cathedral, and crosses. Drive to Crosby,

© 1896 Nature Publishing Group